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Oaksford Chater's theory of reasoning: High prior, lower posterior plausibility

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$$fDiff\,(T1) = \left|\; P(T1=p \mid D=d1) - P(T1=p \mid D=d2) \;\right|$$

Figure 3.

and ordering the test with the highest fDiff.

Thus, Skov and Sherman and Slowiaczek et al. concluded that many subjects use a suboptimal heuristic strategy that is highly correlated with the optimal strategy. Remarkably, however, both the claims (1) that Bayesian diagnosticity (and/or log diagnosticity) are theoretically optimal, and (2) that the feature difference strategy only imperfectly approximates optimal behavior, are in disrepute.

Both expected Bayesian diagnosticity and expected log diagnosticity are poorly behaved as optimal models. To illustrate, suppose that Test 1 were positive in 99% of people with Disease 1, and in 100% of the people with Disease 2. Suppose further that Test 2 were positive in 1% of people with Disease 1, and 99% of people with Disease 2. Test 1 leads, on average, to 50.5% probability of identifying the correct disease; Test 2 leads, on average, to 99% probability of correctly identifying the true disease. Clearly, Test 2 would be more helpful than Test 1 to differentiate between the diseases. Yet diagnosticity and log diagnosticity maintain that Test 1 is infinitely more useful than Test 2! Both diagnosticity measures hold that any test that offers greater-than-zero probability of obtaining 100% certainty of the true disease is infinitely useful. This bizarre claim is not a desirable property of an "optimal" model. (In Nelson [2005; 2008] I discuss these and other theoretical flaws with the diagnosticity measures, and how redefining a single point cannot fix them.)

Better-motivated theoretical models of the value of information, such as information gain-KL distance (Lindley 1956; Oaksford & Chater 1994), probability gain (error reduction; cf. Baron's 1981 talk at the Psychonomic Society Meeting, as cited in Baron 1985), and impact (Klayman & Ha 1987, pp. 219–20; Nelson 2008; Nickerson 1996; Wells & Lindsay 1980) behave reasonably in this medical diagnosis scenario, and do not suffer from the diagnosticity measures' aforementioned theoretical flaws.

Does the feature difference strategy also approximate these better-motivated theoretical models? In fact, it *exactly* corresponds to impact! The highest fDiff feature also has the highest impact, irrespective of the prior probabilities of the diseases and the specific feature probabilities (Nelson 2005, footnote 2).

Closer analysis of the supposedly optimal theoretical models used by some experimenters, and the supposedly suboptimal heuristics used by some subjects, showed that the subjects' heuristic strategy corresponds to a normative model (impact) that is theoretically superior to the normative model that the experimenters had in mind! Put in the context of Marr's (1982) levels of analysis, consideration of subjects' behavior at the algorithmic level can inform thinking about the kinds of computational-level models (normative theories) that are most appropriate (also see Chater et al. 2003; Cohen 1981).

Do all subjects use the feature difference strategy? No. As O&C discuss, the means with which information is presented is important. Different people use a variety of strategies, especially when environmental probabilities are presented in the *standard probability format*, with explicit prior probabilities and likelihoods. The standard probability format is not the most meaningful to subjects; frequency formats better facilitate Bayesian reasoning (Cosmides & Tooby 1996; Gigerenzer & Hoffrage 1995). Personal experience of environmental probabilities may be even more effective. When environmental probabilities are learned through personal experience, the vast majority of subjects maximize the probability of a correct guess (*probability gain*), rather than impact or information gain (Nelson et al., submitted). Note that impact (which the feature difference strategy implements) more reliably approximates probability gain than do Bayesian diagnosticity or log diagnosticity (Nelson 2005), and impact is easily calculated when the standard probability format is used.

Is cognition optimal? Adaptation can be impressive. Insects' flight length distributions appear well-calibrated to natural environments (Viswanathan et al. 1999). But the modern world is evolutionarily novel. For instance, sugar, fat, and salt are available in unprecedented abundance. Similarly, modern media may exaggerate the incidence of plane crashes versus car crashes, or terrorism versus heart disease. The increasing rate of human genetic evolution (Hawks et al. 2007) may facilitate adaptation to some modern environments, over phylogenetic time.

Among topics of interest in Bayesian and rational analysis, such as perception (e.g., Hoffman, in press), memory, information search, and category formation, the correct function to optimize is seldom clear. Baron (2004) noted that utilities are formed on the basis of reflection, and are constantly being modified. As a pragmatic matter, cognitive science would be wise to treat candidate normative models in similar fashion (also see McKenzie 2003). When there are clear and robust discrepancies between human behavior and a particular theoretical model, the normative status of the theoretical model should be reconsidered, as well as the rationality or adaptiveness of the human behavior.

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Oaksford & Chater's theory of reasoning: High prior, lower posterior plausibility

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Abstract: Oaksford & Chater (O&C) subscribe to the view that a conditional expresses a high conditional probability of the consequent, given the antecedent, but they model conditionals as expressing a dependency between antecedent and consequent. Therefore, their model is inconsistent with their theoretical commitment. The model is also inconsistent with some findings on how people interpret conditionals and how they reason from them.

In *Bayesian Rationality* (Oaksford & Chater 2007, henceforth *BR*) the authors present a strong theoretical case for the "probabilistic turn" in the psychology of reasoning. I agree with much of the general thesis of the book: People often reason from uncertain information, and they do so by drawing on probabilistic information. Conditionals, which form the backbone of much of our knowledge, express conditional probabilities. I disagree with Oaksford & Chater (O&C), however, in details of their models of how people reason, and I am less sanguine about the evidence supporting these models. I focus on reasoning with conditionals.

O&C's model of reasoning from conditionals is based on a contingency table of the antecedent (A) and the consequent (C). One axiom of their model is that the marginal probabilities, P(A) and P(C), must be constant when the degree of belief in the conditional changes. This is an unfortunate assumption, for two reasons. First, it is implausible. Assume a new drug X is tested, and it turns out that it causes headaches. Thus, we increase our belief in "If a person takes X then they get a headache." To accommodate the increase in P(headache|X) in one's subjective contingency table, one can either revise P(headache| ¬ X) down to hold P(headache) constant, or else revise P(headache) up while holding P(headache|¬X) constant. The latter appears more reasonable – as drug X is taken by more people, the overall rate of headaches will increase, but the probability of headaches in those who refrain from taking X will not change. Revising P(headache|¬X) down would lead to the absurd conclusion that, when many people take X, those who don't will benefit because they get fewer headaches.

Second, holding P(C) constant links the conditional to the probabilistic contrast, that is, the difference between P(C|A) and P(C|¬A). With P(C) held constant, every increase in belief in the conditional, that is, every increase in P(C|A), must be accompanied by a decrease in P(C|¬A), resulting in an increased probabilistic contrast. As a consequence, there is an ambiguity in O&C's model on what a conditional means. Initially, O&C endorse "the Equation," that is, the probability of "If A then C" equals P(C|A), and is independent of P(C|¬A). But later, O&C seem to endorse the view that a conditional is believable to the degree that the probabilistic contrast is high. For instance, they argue that "it is possible to believe a rule strongly that has many exceptions" (*BR*, p. 190), as long as the probabilistic contrast is high, such as "If a child walks home from school, it is abducted." In line with this reasoning, O&C introduce the "independence model" as the alternative to a conditional hypothesis. The independence model means that $P(C|A) = P(C)$, which implies that the probabilistic contrast is zero. Since the independence model is meant to be the alternative to the conditional, they cannot both have high probability. If the conditional is defined by the Equation, however, P(C|A) can be high and at the same time be equal to P(C). For example, the probability of arriving safely on a flight, given one has a window seat, is very high, but not different from the unconditional probability of arriving safely. It follows that the independence model cannot, in general, be the alternative hypothesis to a conditional when the latter is defined by the Equation.

My colleagues and I tested whether people interpret conditionals as simply expressing a high P(C|A) or as expressing a high probabilistic contrast. We found that people's degree of belief in a conditional depended only on P(C|A), not on P(C|¬A), in agreement with the Equation but not with the probabilistic contrast model (Oberauer et al. 2007). This finding demands a revision of O&C's argument in defence of the MP-MT asymmetry (i.e., the finding that people endorse modus ponens more readily than modus tollens) and their explanation of the Wason selection task, which both assume that the independence model is the alternative to the conditional hypothesis.

The evidence for O&C's model of reasoning with conditionals is mixed at best. Evidence comes from three sources: (1) The model fits endorsement rates for the four basic inference forms. Fitting four data points with three free parameters is no convincing accomplishment, though. A richer database is provided by the frequencies of the 16 possible patterns of endorsement or rejection across the four inference forms. I applied seven formal models of reasoning to such pattern frequencies (Oberauer 2006). O&C's model (Oaksford et al. 2000) provided fits that were worse than all competitors. (2) The model can explain established findings such as negation and suppression effects. Other theories, however, can also explain these effects (Evans & Handley 1999; Markovits & Barrouillet 2002). Therefore, these findings do not support O&C's model over alternatives. (3) Direct manipulation of probabilities is arguably the most direct and stringent test, because no competing theory predicts the same effects as the O&C model. There are two series of experiments using this method. One provided support for the O&C model (Oaksford et al. 2000), whereas the other did not (Oberauer et al. 2004). O&C dismiss the latter evidence as difficult to interpret in light of "the large number of findings showing probabilistic effects in the conditional-inference task and in the selection task" (*BR*, p. 204). At least for the inference task, I fail to see this large number of confirmatory findings.

One difference between the experiments of Oaksford et al. (2000) and those of Oberauer et al. (2004) is that we used the standard deductive instruction, asking participants to judge whether the conclusion follows with logical necessity from the premises, whereas Oaksford et al. simply asked whether one can draw the conclusion. This difference points to a distinction between goals of reasoning, which I think is not sufficiently acknowledged by O&C. The goal of deductive reasoning is to evaluate whether an inference is valid, and in experiments investigating deduction people are instructed accordingly. Most experiments cited in support of probabilistic theories of reasoning, however, ask people to evaluate the soundness of inferences or the truth of conclusions. The (sparse) evidence from direct manipulations of probabilities suggests that people can ignore probabilities when asked to judge validity, whereas they draw on probabilities when asked to rate whether the conclusion is true. Such a modulation of reasoning processes by goals would be entirely rational.

To conclude, the probabilistic view on human reasoning has high a priori plausibility, but the version fleshed out by O&C is conceptually ambiguous and not well supported by the data.

Human reasoning includes a mental logic

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Abstract: Oaksford & Chater (O&C) have rejected logic in favor of probability theory for reasons that are irrelevant to mental-logic theory, because mental-logic theory differs from standard logic in significant ways. Similar to O&C, mental-logic theory rejects the use of the material conditional and deals with the completeness problem by limiting the scope of its procedures to local sets of propositions.

In *Bayesian Rationality* (Oaksford & Chater 2007, henceforth *BR*) the authors reject the conception of human reasoning that focuses on logical inferences, arguing that probability theory should be used instead to account for rationality. Given space limitations, I here address only the two most prominent reasons Oaksford & Chater (O&C) present to reject logic, arguing that they fail to appreciate what mental-logic theory actually proposes.

First, mental-logic theory (e.g., Braine 1990; Braine & O'Brien 1991; 1998; O'Brien 1993; 2004; O'Brien & Manfrinati, in press) consistently has proposed that mental logic differs from standard logic. O&C equate the logical view of conditionals with the truth table for the material conditional (*if p then q* is true unless *p* is true and *q* is false). Indeed, Oaksford and Chater (2003a) stated that the Braine and O'Brien theory includes the material conditional for *if p then q*. The problem with their criticism is that Braine and O'Brien consistently argued that the material conditional does not capture psychological reality. Our theory of conditionals consists instead of two schemas: one for modus ponens (MP) and another for conditional proof. The conditional proof schema states that to derive or evaluate *if p then q*, first suppose *p*; when *q* follows from the supposition of *p* together with other information assumed, one may assert *if p then q*. The schema is applied with a reasoning program that supposes *p* and then treats *q* as a tentative conclusion to be evaluated. When one evaluates a conditional *if not p then not q* from the premise *p or q*, one concludes that the conditional is false, even though this evaluation would not follow when treating *if* as the material conditional (because *p* might be false). Thus,